

October 5, 1953

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Dear Gebe:

I have your letter of September 28, and am sending a sub. of strain K-12 under separate cover.

May I also intrude a caveat, which may be baseless, and which I am somewhat hesitant to bring up except that I am sure you will know how to evaluate it, and will keep it strictly personal. The work with which Mrs. Berry was connected at Amherst (and for which she is presumably responsible to the extent of authorship on the papers) has suffered from grave technical mishaps—reflected for example in the mixed cultures which Plough finally "explained" (rather reluctantly, but under the strongest pressure from other workers) in P.N.A.S. 37:640 and also in the inconsistencies in the paper in P.N.A.S. 38:797; compare with Microb. Genetics Bull. Febr. 1953 p.17, and also his private statement that most of the polyauxotrophic transductions cannot be "consistently" obtained and many no longer react as before. I do not mean to imply that anything more is required than a careful eye to technique. I suspect that many of the inconsistencies may stem from the "reverse auxanographic" method of testing nutritional requirements. Mutagenic effects are some of the most difficult to evaluate, and more than usual attention has to be paid to experimental design and its execution in this field.

Sincerely,

Joshua Lederberg